8

Recommended Flight Investigations and Supporting Ground-Based Activities: 2003-2013

Earlier chapters present evidence of the dramatic scope of NASA's Solar System Exploration program, evidence of the program's remarkable achievements as well as of its weaknesses, and descriptions of the remarkable breadth of the flight and Earth-based opportunities that currently exist to advance solar system science. Since it is an incontestable fact of budgetary constraints that not all these opportunities can be acted upon in the coming decade, a strategy is required that integrates the goals of the diverse elements of the program, moves to strengthen areas of weakness, and accomplishes what can be done through opportunities with both the highest scientific merit and technical readiness.

JUDGING MISSION AND RELATED PRIORITIES

The letter requesting this study called for the generation of a prioritized list of the most promising avenues for flight investigations and supporting ground-based activities. This chapter is devoted to that task. A prioritized list implies that the elements of the list have been judged and ordered with respect to a set of relevant criteria. Exactly the same criteria are used here that were used in Chapter 7 to isolate key scientific questions for the next decade: scientific merit, opportunity, and technological readiness. An assessment of *all* of these criteria together is the essential consideration in determining mission priorities. For example, it would make little sense to have as a first priority a flight mission or ground-based system that was awaiting some long-term technical development or for which no flight or budgetary opportunity existed, no matter how high the scientific merit was rated.

UNDERLYING PROGRAMMATIC REQUIREMENTS

So far, priorities have been discussed in relationship to either scientific questions or specific projects. However, programmatic requirements also need to be considered in building a truly integrated strategy. Individual flight projects recommended for the next decade rest on the base of the long-term program. The top-level programmatic priorities provide the foundation for productivity and continued excellence in planetary exploration and build on the positive aspects of the President's proposed FY 2003 budget for NASA.[1] These priorities are as follows:

1. Continue approved missions, such as the Cassini-Huygens mission to Saturn and Titan and those in the Mars Exploration Program (MEP) and the Discovery program of low-cost missions, and ensure a level of funding that is adequate both for successful operations and for the analysis of the data and publication of the results of these missions. Fundamental research programs, follow-on data-analysis programs, and technology-development programs that support these missions should also be assured adequate funding.

2. Increase the fundamental research and analysis grant programs at a rate above inflation for a decade until they are at a level consistent with the recent change in character of the Solar System Exploration program—that is, a change in the flight rate from a few large missions per decade to one or more small missions per year.

3. Establish the New Frontiers line of principal-investigator-led, competitively procured, medium-cost flight missions applicable to targets throughout the entire solar system and with a total mission cost cap at $650 million.

4. Continue the development and implementation of Flagship missions (e.g., Viking, Voyager, Galileo, Cassini-Huygens) for the comprehensive exploration of extraordinary, high-priority science targets at a rate of roughly one per decade.

5. Continue to support and upgrade the technical expertise and the infrastructure in implementing organizations that provide vital services to enable and support solar system exploration missions.

6. Continue to encourage and participate in international solar system exploration flight programs. Solar system exploration is an inherently international venture, and the U.S. program can benefit from joint ventures.

MISSION LINES AND COMPETITION

The success of the Discovery program, exemplified by the Near-Earth Asteroid Rendezvous (NEAR) mission, Lunar Prospector, and Mars Pathfinder, has convinced even the most hardened skeptic that small, relatively low-cost missions can effectively address significant scientific goals. The discipline of Discovery's competitive selection process has been particularly effective in eliminating ill-conceived concepts and has resulted in a richness of mission goals that few would have thought possible a decade ago. The planetary science community's enthusiastic support for Discovery has led to calls for the competitive acquisition of all flight projects. The experience during the past decade in developing mission concepts (i.e., various Pluto flyby and Europa orbiter mission concepts) for which traditional procedures have led to escalating cost estimates has amplified this call. The proposed line of New Frontiers missions is specifically intended to be competitively selected. Competition is seen as a vehicle to increase the scientific richness of flight missions and, perhaps of equal importance, as a device to constrain the large costs associated with flying robotic missions to the planets.

Because of the positive experience with Discovery and also because of NASA's recent success in competing an outer solar system mission in the New Frontiers cost category, **the SSE Survey strongly endorses the New Frontiers initiative. These spacecraft should be competitively procured and should have flights every 2 or 3 years, with the total cost capped at approximately twice that of a Discovery mission. Target selection should be guided by the list in this report.**

While competitive selection has its advantages, its negative aspects should also be taken into consideration, and avoided if possible. They are as follows:

- *Competition leads to secrecy in the conceptual phase of a mission.* For small missions having an adequate number of scientifically focused flight opportunities, this does not seem to be a demerit. However, with intrinsically expensive missions for which the flight opportunities may be singular and the scientific goals broad, it can be a problem. For New Frontiers missions, it does not seem advisable for conceptual scientific development to become the responsibility of a narrowly focused group in the community, no matter how well motivated they are. The selection of New Frontiers missions needs to be a continuing process involving broad community input, as has been accomplished by this decadal survey report.
- *Competition for New Frontiers missions may lead to a substantial increase in the overall costs associated with conceptual mission development during the preselection stage.* As yet, the SSE Survey knows of no estimate or clearly identified source of funds for the development of proposals for New Frontiers missions. The cost of developing a Discovery proposal to the final stage of a competition is not negligible. These costs can be expected

to increase with the size and scope of the mission. The cost to develop a New Frontiers mission proposal will be considerably more than for Discovery missions. In Discovery, these funds come partly from the overhead charged on other projects at an implementing institution and partly from NASA (particularly in the final stages of the competition). **The SSE Survey recommends an early study to determine the means for providing the funds necessary to underwrite proposal competition in New Frontiers missions.**

• *Competition may lead to conflicts of interest at NASA centers.* There are areas of unique expertise resident in single NASA centers that must be supported and maintained as necessary and required to carry out the planetary exploration enterprise (e.g., mission analysis, navigation, and deep-space communications). This expertise is often supported from institutional overhead on ongoing center missions. Since these same centers may also wish to compete, particularly for large missions, the centers will face a conflict of interest when deciding whether to make such unique services available to their competitors. **The SSE Survey recommends an early study to find ways to avoid the potentially adverse consequences of conflicts of interest relating to, for example, access to unique expertise and infrastructure at NASA centers.**

DEFINITION OF MISSION COST CLASSES

In the discussion of mission priorities that follows, The SSE Survey, at NASA's explicit request, divided missions into classes on the basis of anticipated total mission cost to completion (but without extension). The mission cost classes adopted are as follows:

- Small—less than $325 million,
- Medium—between $325 million and $650 million, and
- Large—more than $650 million.

For example, a Discovery or Mars Scout mission is a small mission by definition. New Frontier missions, as defined in the President's proposed FY 2003 budget, are equivalent to the Survey's medium-mission category. Flagship missions, for example, Europa Geophysical Explorer or Mars Sample Return, are in the large-mission category. The SSE Survey used the best information available to it in assigning cost categories to the mission concepts evaluated in this survey. Nevertheless, it must be emphasized that the cost estimates, particularly for the New Frontiers missions, are based on concept studies of limited scope. **In order to confirm the readiness of any New Frontier mission concept prior to the issuance of an Announcement of Opportunity and to certify the mission concept's qualification for this program, the SSE Survey recommends that after the first selection, an independent group conduct a certification review of the mission concept to be solicited, prior to the issuance of any Announcement of Opportunity.**

SMALL MISSIONS

The Discovery Program

The Discovery line of small missions is reserved for competed missions responsive to discoveries and is outside the context of any long-term strategy. Over the course of any 10-year period, there are certain to be new discoveries and high-science-value mission ideas that could not be discerned at the beginning of the strategic planning period. The Discovery program provides for the necessary flight program flexibility to cover these contingencies and to provide continuing new opportunities to the planetary science community for mission ideas not provided in the long-term strategic plan. The Discovery program is fundamental and invaluable for planetary exploration, but it is outside the bounds of this long-term strategic plan. Therefore, the SSE Survey makes no specific flight mission recommendations for the Discovery program, but it is compelled to make a recommendation on the value of these missions to planetary exploration**. Given Discovery's highly successful start, the SSE Survey endorses the continuation of this program, which relies on principal-investigator leadership and**

competition to obtain the greatest science return within a cost cap. A flight rate of no less than one launch every 18 months is recommended.

Flight Mission Extensions

The SSE Survey recognizes mission extensions, even multiple extensions, as significant and highly productive elements both of nominally successful missions and of missions that undergo changes of scope or time lines due to unpredictable events. The Voyager extensions to Neptune, Uranus, and the outer heliosphere are examples of the former, and the NEAR extension at Eros and the Galileo Europa/Millennium and Deep Space 1 extensions are highly productive examples of the latter. The Survey treats these extensions, which it asserts will require their own funding arrangements, as independent, small-class missions. The Discovery program can make decisions on mission extensions within the Discovery program line by trading off Announcement of Opportunity release dates. As the examples cited above indicate, the productivity and effectiveness of mission extensions in solar system exploration are unquestionable and constitute an important part of the Survey's integrated strategy. **The SSE Survey supports NASA's current Senior Review process for deciding the scientific merits of a proposed mission extension and recommends that early planning be done to provide adequate funding of mission extensions, particularly Flagship missions and missions with international partners.**

PRIORITIZED FLIGHT MISSIONS FOR THE DECADE 2003-2013

The mission concepts proposed by the SSE Survey's panels (see Part One) as future flight mission candidates are compiled in Table 7.1 in Chapter 7. They encompass missions to a diverse set of targets, from Mercury to beyond the orbit of Pluto. These concepts touch on a broad range of questions that include the formation of the solar system, the evolution of habitable worlds, the origin of life, and the fate of Earth. Some of these missions can be flown with proven technology; others require substantial technological development. It is clear that, given their cost implications, not all of the missions listed in Table 7.1 can be recommended for flight in the next decade, and therefore the SSE Survey prioritized them.

To form a scientific basis for its integrated strategy (see Chapter 7), the SSE Survey used the criteria of scientific merit, opportunity, and technological readiness to isolate 12 key scientific questions to be addressed during the next decade. It then showed how these questions relate to a small set of mission candidates, highlighted in bold type in Table 7.1, which are the mission set from which the Survey created its prioritized list of missions suitable for flight in the next 10 years.

Overall program cost constraints are a fact of life. The SSE Survey restricted the number of missions in its prioritized list to a number that it believes can be accommodated within the out-year budget profile in the President's proposed FY 2003 budget: for large-class missions, the number is limited to one, and for medium-class, the number is limited to three; these are supplemented by two extra mission candidates to account for uncertainties, to encourage further possible growth in the program, and also to give some indication of the possible direction for the program beyond the current decade. The SSE Survey's recommendations for non-Mars missions, therefore, consist of a prioritized list of five medium-class missions for the New Frontiers program, the start of one large-class mission during the decade, and one small-class non-Discovery mission extension.

Many discoveries occur in the planetary sciences over the course of a decade, and for a decadal strategy to maintain a course consistent with ongoing discoveries, the need to reconsider the priorities recommended by this Survey may arise. NASA should issue Announcements of Opportunity for New Frontiers missions that are consistent with the priorities given in this Survey. Only in the case where a new discovery changes the Survey's fundamental understanding should these priorities be reconsidered, in which case **the SSE Survey recommends that the National Research Council's Committee on Planetary and Lunar Exploration conduct a review to confirm or modify decadal survey recommendations and priorities for the New Frontiers flight program.**

The number of Discovery missions is constrained only by the funding profile. **Recognizing the Discovery program's success, the SSE Survey recommends that adequate resources be provided to sustain an average flight rate of no less than one launch every 18 months.**

While the Discovery program has resulted in great success for small missions and the New Frontiers program holds great promise for moderate-cost missions, some high-priority science investigations will require higher-cost missions. **The SSE Survey recommends that Flagship (>$650 million) missions be developed and flown at a rate of about one per decade. In addition, for large missions of such inclusive scientific breadth, a broad cross section of the community should be involved in the early planning stages.** Future survey committees should have at their disposal well-developed planning studies for missions in this class in order to make sensible decisions on prioritization. **The SSE Survey recommends that NASA conduct a series of advanced studies of Flagship mission concepts with broad community participation over each 10-year period prior to decadal surveys**. These advanced studies could be selected through a competitive process analogous to the 21st Century Mission Concepts for Astrophysics program run by NASA in the mid-1990s "to solicit innovative proposals for concept studies of new flight missions which can enhance capabilities for frontier research. . ." and "develop a menu of potential new mission concepts to be considered for the next decadal survey committee."[a]

The rationale for the SSE Survey's prioritization within the Mars Exploration Program, which places a high priority on an early Mars Sample Return mission, is treated separately below. The final prioritized list of flight-mission candidates is shown Table 8.1. As indicated, the ranking reflects the Survey's assessment of the scientific merit, technological readiness, and special opportunities associated with each mission.

Scientific Rationale for Priorities in the Medium-Class New Frontier Line

Kuiper Belt-Pluto Explorer

A mission to the Kuiper Belt, including Pluto-Charon, will provide the first exploration of this newly discovered domain in the solar system, provide important insights into the physical nature of these planetary building blocks, and allow us to survey the organic matter and volatiles that they contain. Collisions with objects such as these diverted into the inner solar system may have imported the basic volatile and molecular stock from which habitable environments were constructed in early planetary history. Little is known of the physical properties of Kuiper Belt objects (KBOs). However, what is known (several physically large objects with high rates of spin, several loosely bound binaries, and a wide range of color) indicates that they have diverse and unexpected properties. The value of this mission increases as it observes more KBOs and investigates the diversity of their properties. The SSE Survey anticipates that the information returned from this mission might lead to a new paradigm for the origin and evolution of these objects and their significance in the evolution of objects in other parts of the solar system.

Comparison of the cratering records on Pluto, Charon, and several smaller objects at a range of heliocentric distances will provide our first data on the collisional history of this region. Comparison of the surface compositions of objects in the belt with Pluto and Charon and Triton may allow us to separate evolutionary surface processes from primordial surface properties in the outer solar system. The observations, if extended to small objects, may provide information on whether comets are collisional fragments from large KBOs or are themselves primordial bodies. The surface material on KBOs may not survive entry into the inner solar system. Investigation of the composition of this material, which is probably the most primitive in the solar system, will provide an important reference for comparison with the surface materials on related bodies, including the Centaurs, the nuclei of comets, and certain near-Earth asteroids. The technical readiness of this mission is judged high, owing to the ongoing development of a technically equivalent mission concept.

[a]Michael Kaplan, NASA Headquarters, presentation to the National Research Council's Task Group on Space Astronomy and Astrophysics, March, 1996, background materials compiled by Shobita Partnasarathy and David H. Smith.

TABLE 8.1 An Integrated Strategy for Solar System Exploration: Prioritized List of Flight Missions for the Decade 2003-2013

Mission List		Science				Technology and Opportunity	
Rank in Cost Class	Mission Concept Name	Could Create New Paradigm	Could Change Existing Paradigm	Results Will Be Pivotal	Will Add to Factual Base	Technical Readiness	Special Opportunities
SOLAR SYSTEM FLIGHT MISSIONS (non-Mars)							
Small							
1	Cassini Extended	x	xx	xxx	xxx	xxx	1
Medium							
1	Kuiper Belt-Pluto Explorer	xxx	xxx	xxx	xxx	xxx	1
2	South Pole-Aitken Basin Sample Return	xx	xx	xxx	xxx	xx	3
3	Jupiter Polar Orbiter with Probes	xx	xxx	xxx	xxx	x	
4	Venus In Situ Explorer	x	xxx	xxx	xxx	x	
5	Comet Surface Sample Return	xxx	xxx	xxx	xxx		
Large							
1	Europa Geophysical Explorer	xxx	xxx	xxx	xxx	xx	
MARS FLIGHT MISSIONS (beyond 2005)							
Small							
1	Mars Scout line	x	xx	xxx	xxx	xxx	1
2	Mars Upper Atmosphere Orbiter	x	xx	xxx	xxx	xx	2
Medium							
1	Mars Science Laboratory	x	xx	xxx	xxx	x	1
2	Mars Long-Lived Lander Network	xx	xxx	xxx	xxx	x	2
Large							
1	Mars Sample Return	xxx	xxx	xxx	xxx		2
DISCOVERY FLIGHT MISSIONS							
One launch every 18 months							

NOTE: Science and technology evaluation codes: xxx, high; xx, medium; x, modest.
Opportunity codes: 1, approved mission, operating spacecraft or celestial mechanics; 2, international; 3, technology opportunity.

Lunar South Pole-Aitken Basin Sample Return

The goal of the South Pole-Aitken Basin Sample Return (SPA-SR) mission is to understand the nature of the Moon's upper mantle and to tie down early impact chronology by returning samples from the South Pole-Aitken Basin. This basin is the largest known in the solar system and is stratigraphically the oldest and deepest impact structure preserved on the Moon. This giant excavation penetrates the lunar crust and allows access to materials from the upper mantle, and so may have a substantial effect on our current paradigm for the differentiation process. Absolute dating of returned samples, which will include both soil and diverse rock chips, could also change our understanding of the timing and intensity of the late heavy bombardment suffered by both the early Earth and Moon. The emergence of life on Earth that was ancestral to our contemporary biosphere could not have occurred until after the last global, total sterilization impact event, which likely corresponds to the end of the period of heavy bombardment.

A sample-return mission such as SPA-SR—that is, one of moderate technical difficulty—is an opportunity to gain relevant experience for much more complex sample-return missions from Mars and from Venus.

Jupiter Polar Orbiter with Probes

There are five primary objectives for the Jupiter Polar Orbiter with Probes (JPOP) mission. First, it will determine if Jupiter has a core, a question that is key to giant planet formation. One theory holds that a rock-ice "seed" of some 10 Earth masses is necessary to attract the lighter gases hydrogen and helium. Another theory says that Jupiter-sized objects can form as stars do, attracting gas, ice, and dust directly from the nebula.

Second, JPOP will measure the water abundance (hence, the O/H ratio), which is uncertain by an order of magnitude even though oxygen is expected to be the third-most-abundant element after hydrogen and helium. Water plays an important role in giant planet formation. The O/H ratio tells us how giant planets got their volatiles (H_2O, CH_4, NH_3, and H_2S) and, in particular, the extent to which the volatiles were carried from beyond Neptune's orbit to the inner solar system on icy planetesimals.

Third, JPOP will measure the deep winds to 100 bars and will give some information about the winds to thousands of bars. The deep winds may be key to the extreme stability of the weather systems observed at cloud top.

Fourth, by virtue of its cloud-skimming orbit, JPOP will measure the higher harmonics of the magnetic field, which is key to understanding how Jupiter's dynamo works.

Fifth, JPOP will repeatedly visit the hitherto unexplored polar magnetosphere, where the currents that maintain corotation (of the plasma with the planet) pass into the atmosphere and cause the jovian aurorae.

Venus In Situ Explorer

The Venus In Situ Explorer (VISE) mission is a detailed exploration and study of the composition of Venus's atmosphere and surface materials. Venus and Earth may have had very similar surface conditions early in their histories, but Venus's subsequent evolution was different from Earth's, developing an environment unsuitable for life. However, Venus is still a dynamic world with active geochemical cycles and nonequilibrium environments in the clouds and near surface that are not understood. VISE will make compositional and isotopic measurements of the atmosphere on descent and of the surface on landing. A core sample is obtained at the surface and lofted to altitude where further geochemical and mineralogical analyses are made. In situ measurements of winds and radiometry are obtained during descent and at the balloon station. Scientific data obtained by this mission would help to constrain the history and stability of the Venus greenhouse and the recent geologic history, including resurfacing. The technology development achieved for this mission will pave the way for a potentially paradigm-altering sample-return mission in the following decade.

Comet Surface Sample Return

A first sample from the near-surface layer of a comet, if taken from an active area (perhaps at sunrise when activity is low) will provide the first direct evidence on how cometary activity is driven (whether the water is very close to the surface). The Comet Surface Sample Return (CSSR) mission would provide the first real data on how small bodies accrete (physical structure at scales from microscopic to centimeters), chemical resolution of the organics in the wealth of large-mass molecules and fragments seen at Halley, and the first direct data on the selection effects that operate between the nucleus and the relatively well-studied material in cometary comae.

CSSR will also provide invaluable information on how the particles on a cometary nucleus are bound together: Is there an organic glue? Is there contact welding? It will also provide the first direct information on the scales of physical and compositional heterogeneity: Is it microscopic as seen in meteorites, or is cometary material homogeneous at the microscopic scale? Finally, CSSR will provide the first information on the macroscopic mineralogical and crystalline structure and isotopic ratios in cometary solids and also the first information on the physical relationships between volatiles, ice, refractory material, and its porosity.

Scientific Rationale for Large-Class Missions Outside the Mars Exploration Program

One Flagship mission is recommended for this decade—the Europa Geophysical Explorer.

Europa Geophysical Explorer

Europa holds the most promise for increasing current understanding of the biological potential of icy satellites. Convincing evidence exists for the presence of liquid water within just tens of kilometers of the surface, and there is evidence for recent transfer of material between the surface and the water layer. Europa's ocean is probably in direct contact with a rocky mantle below and is potentially endowed with hydrothermal systems, so chemical disequilibrium may be able to nourish oceanic organisms. The first step in understanding the potential for icy satellites as abodes for life is a Europa mission with the goal of confirming the presence of an interior ocean, characterizing the satellite's ice shell, and understanding its geological history. Europa is important for addressing the issue of how far organic chemistry goes toward life in extreme environments and the question of how tidal heating can affect the evolution of worlds. Europa is key to understanding the origin and evolution of water-rich environments in icy satellites. **The SSE Survey endorses the current recommendations for a mission to orbit Europa. However, given the high cost of the Europa Geophysical Explorer mission, the Survey considers it essential that the mission address both the Group 1 and Group 2 science objectives described by the Europa Orbiter Science Definition Team.** These objectives are as follows:

- *Group 1.* Determine the presence or absence of an ocean; characterize the three-dimensional distribution of any subsurface liquid water and its overlying ice layer; and understand the formation of surface features, including sites of recent or current activity, and identify candidate landing sites for future lander missions.
- *Group 2.* Characterize the surface composition, especially compounds of interest to prebiotic chemistry; map the distribution of important constituents on the surface; and characterize the radiation environment in order to reduce the uncertainty for future missions, especially landers.

Flagship missions have been a traditional means for international cooperation in which NASA and other national space agencies, including the European Space Agency (ESA), can leverage their resources to accomplish what might otherwise be difficult to achieve. Galileo and Cassini-Huygens provide good examples in this respect, and **the SSE Survey recommends that NASA engage prospective international partners in the planning and implementation of the Europa Geophysical Explorer.**

Relative Priorities Between Mission Cost Classes

The SSE Survey did not attempt to prioritize across mission cost classes so that flexibility is preserved in order to address opportunities in the annual budget cycle. The opportunities to mount large-class missions are very limited, and if a lower-cost mission can be accommodated in a new budget cycle, it should not be thwarted by a requirement to wait for an opportunity to initiate a more expensive mission. **Rather than compete large-class missions with missions in other cost classes, the SSE Survey recommends flying large-class missions at an appropriate frequency (i.e., roughly one per decade), independent of the issues facing new starts in other cost classes.**

Since large-class missions represent an enormous investment and generally require a decade of study to mature in concept and design, **the SSE Survey recommends that NASA establish a procedure for reevaluating the candidate list of large-class missions for the decade 2013-2023. Two possible mechanisms for this procedure include (1) the appointment of a Science Definition Team every 3 years to define candidate missions or (2) a periodic competition for funds to support initial definition studies of missions concepts.** Some large-class missions identified by the SSE Survey for the 2013-2023 decade, and which should be revisited in the near future, are listed in Box 8.1.

BOX 8.1
Deferred High-Priority Flight Missions

The SSE Survey deemed the following mission concepts worthy of flight and accorded them a high priority. However, for reasons of mission sequencing, technological readiness, or budget, they did not make the final cut for the coming decade:

Medium Class
Geophysical Network Science
Trojan/Centaur Reconnaissance Flyby
Asteroid Rover/Sample Return
Io Observer
Ganymede Observer

Large Class
Europa Lander
Titan Explorer
Neptune Orbiter with Probes
Neptune Orbiter/Triton Explorer
Uranus Orbiter with Probes
Saturn Ring Observer
Venus Sample Return
Mercury Sample Return
Comet Cryogenic Sample Return

The Kuiper Belt-Pluto Explorer is the first priority in the medium-cost class, and the Europa Geophysical Explorer (EGE) mission is the first priority in the large-cost class. The Kuiper Belt-Pluto Explorer mission, with its potential for creating a new paradigm regarding primitive processes in the outer solar system and their effect on the evolution of bodies in other parts of the solar system, has scientific merit similar to that of the EGE mission, which seeks primarily to define a possible habitat for life by vastly expanding our current knowledge of a subsurface ocean. With respect to technical readiness and special opportunities the Kuiper Belt-Pluto Explorer mission has clear advantages over EGE.

Deferred High-Priority Missions

The prioritization process forces the SSE Survey to defer what would otherwise be excellent high-priority missions worthy of flight. Box 8.1 lists mission concepts that are among the highest-ranked by the SSE Survey's panels, but that did not make the final recommended priority list for the coming decade.

Some of these missions are deferred because their science objectives can be more precisely defined after precursor missions are flown. The Europa Lander should follow as the next step after the Europa Geophysical Explorer. Similarly, a Titan Explorer mission should follow Cassini-Huygens. After having conducted orbiter missions to the two gas giants, Jupiter (Galileo) and Saturn (Cassini), an orbiter mission to an ice giant should follow—the highest-rated being a Neptune orbiter mission carrying deep atmosphere (100-bar) probes with special attention to Triton exploration through flybys and perhaps a lander. These outer-planet missions will be enhanced and enabled by advanced nuclear power and propulsion. Venus sample return should follow after experience with lunar and martian sample return, and a cryogenic comet sample return should follow experience with a non-cryogenic sample-return mission. The proposed set of medium-class/New Frontiers missions should be revisited on an appropriate time scale as new discoveries are made in the course of the solar system exploration enterprise.

PRIORITIES FOR THE MARS EXPLORATION PROGRAM

The exploration and scientific investigation of Mars have reached an important stage. Exciting discoveries from recent successful missions and the ongoing research and analysis of data from these missions and martian meteorites have established a broad understanding of the planet and its evolution. These developments have also raised a number of fundamental and compelling questions related to all aspects of Mars, from the outer atmosphere and space environment to the deep interior. The sheer number of questions presents a challenge to establishing a rationale and a fiscally prudent plan that moves toward addressing the highest-priority question identified by numerous bodies (e.g., COMPLEX, MEPAG, and this survey): Did life ever arise on Mars? No single measurement at a specific location on Mars will answer this question. Nor is the importance of the question understood without a broad understanding of Mars's current processes and past evolution.

It is imperative that the exploration of Mars move aggressively to surface missions for in situ science investigations and that it lay the foundation for sample return, the latter beginning early in the decade 2013-2023. In situ science is progressing rapidly, and such investigations will add substantially to our knowledge across a broad range of disciplines for Mars. However, the results that will flow from the detailed investigations of martian samples returned to Earth using modern techniques and sophisticated equipment will simply dwarf all previous results. It is important to be aware that the first samples returned from Mars may not be definitive regarding the life question, no matter how carefully the samples are selected. However, the first returned samples would establish beyond a shadow of a doubt how the exploration of Mars must proceed and where to explore, using in situ measurements and additional returned samples. Equally importantly, these samples will forever change our understanding of geologic and climate evolution, surface-atmosphere interactions, and Mars as an abode of life.

Table 8.1 above contains the prioritized list of missions for the future Mars Exploration program, and Table 8.2 indicates a possible mission sequence for their implementation.

Recommended Mars Missions

Mars Sample Return

Observations by robotic orbiters and landers alone are not likely to provide an unambiguous answer to the most important questions regarding Mars: whether life ever started on that planet, what the climate history of the planet was, and why Mars evolved so differently from Earth. The definitive answers to these questions will require analysis in Earth-based laboratories of Mars samples returned to Earth from known provenances on Mars. Moreover, samples will provide the ultimate ground-truth for the wealth of data returned from remote-sensing and in situ missions**. The SSE Survey recommends that NASA begin its planning for Mars Sample Return missions so that their implementation can occur early in the decade 2013-2023.**

The Need for Sample Return to Search for Life. At our present state of knowledge and technological expertise, it is unlikely that robotic in situ exploration will be able to prove to an acceptable level of certainty whether there

TABLE 8.2 A Possible Sequence for Future NASA Mars Science Missions with Early Sample Return

Year of Launch				
2005	2007	2009	2011	2014
Mars Reconnaissance Orbiter	Mars Scout 1	Mars Science Laboratory	Mars Scout 2	Mars Sample Return with international partners

once was or is now life on Mars. Results obtained from life-detection experiments carried out by robotic means can be challenged as ambiguous for the following reasons:

- Results interpreted as showing an absence of life will not be accepted because the experiments that yielded them were too geocentric or otherwise inappropriately limited;
- Results consistent with but not definitive regarding the existence of life (e.g., the detection of organic compounds of unknown, either biological or nonbiological, origin) will be regarded as incapable of providing a clearcut answer; and
- Results interpreted as showing the existence of life will be regarded as necessarily suspect, since they might reflect the presence of earthly contaminants rather than of an indigenous martian biota.

The Need for Sample Return for Geochemical Studies and Age Dating. Rocks contain a near-infinite amount of information on a microscopic scale, some of it crucial to an understanding of the rock's origin and history. The constituent minerals, fluid inclusions, and alteration products can be studied chemically and isotopically, providing critical information on the age, dates of thermal and aqueous alteration events, nature of the source regions, and history of magmatic processes. In situ instrumentation will always be limited to a fraction of the potential measurement suite and lower levels of precision and accuracy. Information about the Mars climate will be found in the layer of weathering products that we expect to find on rock samples and in the soils. These products will almost certainly be very complex minerals or amorphous reaction products that will tax our best Earth-based laboratory techniques to understand. A critical unknown for Mars is the absolute chronology of the observed surface units. Precise and accurate dating of surfaces with clearly defined crater ages is best accomplished with returned samples.

The Need for Sample Return for Studies of Climate and Coupled Atmosphere-Surface-Interior Processes. Key measurements in modeling the relative loss of portions of the atmosphere to space and to surface reservoirs are surface mineral compositions and their isotopic systematics. Atmospheric loss processes (e.g., hydrodynamic escape, sputtering) leave characteristic isotopic signatures in certain elements. Loss to space and surface weathering (e.g., CO_2 to carbonate minerals) are likely to produce isotopic fractionation in different directions. $^{15}N/^{14}N$ in the martian atmosphere is understood to have evolved over the past 3.8 billion years (it is currently 1.6 times the terrestrial value), and a determination of this ratio in near-surface materials may constrain the time of their formation. Compositional and isotopic analysis of surface minerals, weathering rinds, and sedimentary deposits will establish the role of liquid water and processes such as weathering. The corresponding measurements on volatiles released from near-surface materials are likely to be more heterogeneous and may provide fossils of past atmospheric and chemical conditions that allow the past climate to be better understood.

The SNC Meteorites Do Not Obviate the Need for Sample-Return Missions. SNC meteorites have provided a tantalizing view of a few martian rocks and a demonstration of how much can be learned when samples can be examined in Earth-based laboratories; however, they represent a highly selected subset of martian materials, specifically, very coherent rocks of largely igneous origin from a small number of unknown locations. Thus, SNC meteorites are unhelpful in answering one of our outstanding questions—What is the absolute chronology of Mars?—because although these meteorites can be accurately dated, the geologic units from which they are derived are unknown. While returned samples are also a selected subset of martian materials, we will know their geologic context, and they will be from sites selected because they can provide particularly valuable information.

Mars Science Laboratory

The Mars Science Laboratory (MSL) is an important mission along the path of "Seek, in situ, and sample." The science goals are to conduct detailed in situ investigations of a site that is a water-modified environment identified from orbital data. As such, this mission will provide critical ground-truth for orbital data and test hypotheses for the formation and composition of water-modified environments identified through morphological and spectroscopic investigations. The types of in situ measurements possible on MSL are wide ranging, including atmospheric sampling, mineralogy and chemical composition, and tests for the presence of organics. There currently is some debate as to whether this mission will have roving capability on the order of 10 km, or be more focused toward drilling to get below the surface, which is hostile to life. Both strategies have merit in addressing high-priority science goals, though the drilling mission puts a much greater demand on precision landing. Regardless of the ultimate design of the instrumentation, **the SSE Survey recommends that while carrying out its science mission, the Mars Science Laboratory mission should test and validate technology required for sample return (e.g., sample handling and storage in preparation for sample return and feed-forward lander design, consistent with the future use of a Mars Ascent Vehicle).** In addition, the surface operations of the Mars Science Laboratory mission should feed forward to Mars Sample Return.

Mars Scout Program

Mars Scout provides an excellent opportunity for NASA to address science priorities outside the principal objectives of the Mars Exploration Program, and for the broad science community to respond to discoveries and technological advancement. **The SSE Survey recommends that the Mars Scout program be managed as is the Discovery program, with principal-investigator leadership and competitive selection of missions.** It is essential, therefore, that the measurement goals for the Mars Scout program be directed toward the highest-priority science for Mars and be selected by peer review. The missions-of-opportunity element of the Scout program is also important, as it allows for participation in foreign Mars missions. **The SSE Survey strongly recommends that the Mars Exploration Program commit equally as strongly to the Scout program as to sample return.**

While Mars sample-return missions will be expensive and consuming of the attention of the MEP, there are sufficient resources in the program as currently structured to achieve both a viable Scout program and sample return. As witnessed by the response to the recent call for Scout proposal ideas (over 40 submissions were received), tremendous enthusiasm has been stimulated by recent Mars discoveries and scientific investigations not covered by the MEP. Scout provides a mission component that is highly flexible and responsive to discovery. **The SSE Survey recommends that a Mars Scout mission be flown at every other launch opportunity.**

Mars Long-Lived Lander Network

The SSE Survey's Mars Panel considers that a long-lived network of landed science investigations (ML^3N) should be a high-priority Mars mission. The principal experiments on these landed stations should be passive seismometers to determine interior structure and activity, and analyzers of the ground-level atmosphere to address areas of importance to martian atmospheric science (meteorology, atmospheric origin and evolution, chemical stability, and atmospheric dynamics). Both the seismological and atmospheric measurements must continue to record data for at least 1 martian year to achieve their potential. NASA advisory panels have consistently recognized the importance of these experiments and recommended their implementation.[2] These questions are of particular interest for a broad community of scientists, because useful comparisons with Earth can be made that may prove important for understanding the atmospheric evolution of both planets. Network science has been identified by the European Space Agency as a priority for Mars (the NetLander mission).

Mars Upper Atmosphere Orbiter

The SSE Survey includes in its priority scheme an orbiter dedicated to studies of Mars's upper atmosphere and plasma environment. Interactions with the solar wind are thought to have played a significant role in the long-term evolution of the martian atmosphere, yet no measurements have been made to confirm or reject these ideas. A variety of atmospheric escape processes have been inferred from indirect measurements and/or predicted from theoretical models. This mission would provide quantitative information on the various potential escape fluxes and, thus, quantify current escape rates. Back extrapolation of such measurements might result in new understanding of the evolution of the martian atmosphere and maybe also provide important clues to atmospheric evolution on Venus and Earth. In carrying out these measurements, numerous other important questions of high scientific value associated with the middle and upper atmosphere, exosphere, ionosphere, and solar-wind interaction processes will also be addressed.

No plans exist in the current U.S. Mars Exploration Program to address any of the scientific questions identified by previous panels in this area. The Nozomi and Mars Express missions will address them to some extent, but much more data will be needed to meaningfully elucidate these issues. The measurements required for this mission could be accommodated as a science package on an international orbiter mission or as a stand-alone mission in the Mars Scout program.

Staging, Sequencing, Links to Other Mars Missions, and International Partnerships

Developed in 1999 after the failures of Mars Polar Lander and Mars Climate Orbiter, the Mars Exploration Program is founded on the pursuit of the highest-priority investigations along the path of “Seek, in situ, and sample.” The “Seek” component consists of orbital investigations to identify sites with remotely sensed signatures indicative of water. The “in situ” component involves getting to the surface for detailed characterization of specific sites and providing ground-truth for orbital measurements. Finally, the “sample” component concerns the return to Earth of pieces of Mars that will be important for addressing the life question as well as all other aspects of martian science.

The MEP plans for a mission to Mars at every launch window (approximately once every 2 years) and is cost-constrained to some $700 million per opportunity. The program is designed to be flexible and responsive to discoveries, though mission design and implementation cycles require that the science objectives and instrument suite for the next opportunity be fixed prior to the results derived from the current opportunity.

The Mars Exploration Program is currently reevaluating future missions, principally in response to the high cost of sample return. The program is being directed to develop discovery-driven investigation pathways with missions at every opportunity, unless compelling scientific justification can be developed for sample return. The SSE Survey believes that sufficient resources exist in the Mars Exploration Program to achieve the highest-priority mission identified by this and other panels (COMPLEX, MEPAG, and so on) while maintaining a flexible and discovery-driven program of Mars exploration. Furthermore, this can be achieved to allow the first sample-return mission early in the next decade (2013-2023). As an example, one possible pathway with an early sample return is outlined in Table 8.2. The interleaving of Mars Scout with other MEP missions maintains the discovery-driven aspects of the program. It is important to recognize that MSR will be a long mission from development, through launch, sample return, and sample analysis. It will take some time after the samples return to Earth for the results of the analyses to be integrated with previous Mars knowledge. Additionally, sample containment and curation facilities must be operational before samples are returned, as was emphasized earlier in this report.

The SSE Survey advocates that MSL be structured to accomplished high-priority science goals and to achieve technological advances necessary for sample return. Sample-return technology can also be leveraged from developments in other missions, most importantly the lunar South Pole-Aitken Basin Sample Return mission, recommended as a priority for the New Frontiers program. There are likely many common elements between this mission and MSR, for example, the ascent vehicle, orbital rendezvous, landing systems, and sample handling and receiving. In fact, the opportunity to test the Earth-return aspects of sample handling without the high-level planetary protection protocols required for MSR might be a critical test of the technologies required for MSR.

Countries other than the United States are keenly interested in Mars exploration and have committed significant resources to national and international programs. Many of these countries have expressed a willingness to participate in NASA’s efforts, and several joint efforts are currently under way. The SSE Survey advocates that NASA actively pursue international collaborations such as Missions of Opportunity on European orbiters and landers. **The SSE Survey recommends that NASA engage prospective international partners in the planning and implementation of Mars Sample Return at an early stage in order for this complex mission to benefit fully from the capabilities and resources offered by the international community.**

ADVANCED TECHNOLOGY

Technology Development

The SSE Survey recommends that NASA commit to significant new investments in advanced technology so that future high-priority flight missions can succeed. Unfortunately, erosion has occurred in the level of investment in technology in the past several years. Flight-development costs have increased over projections, and investments in advanced technologies have been redirected to maintain flight-mission development schedules and performance.

For most of the history of planetary exploration, large-cost flight missions such as Voyager, Viking, Galileo, and Cassini have carried a large portion of the technology-development burden in their development costs. During the change in the last decade to a larger number of lower-cost flight missions, the consequent loss of technology development by large missions was compensated by adding separate technology-development cost lines to the planetary exploration portfolio, such as X2000, under an understood policy of "no mission start before its technological time." This mechanism was intended to separate and remove the uncertainties in technological development from early flight-development costs. However, flight-mission costs have been underestimated, and development plans have been too success-oriented, resulting in erosion of technology-development lines by transfer to flight-development costs. This trend needs to be reversed in order to realize the flight missions recommended in this report.

This report identifies a clear set of missions for development in the next decade, providing a compelling focus for advanced technology development. NASA must maintain this focus, even as it increases competition in technology development, to ensure long-term stability and strong coordination with flight-mission needs.

Generic Technologies

Generic technologies exist that will benefit almost every flight program. To focus technology development on the most important needs for the next decade, the SSE Survey identified the most enabling technologies for key interplanetary spacecraft subsystems—power, propulsion, communication, architecture, avionics, and instrumentation—and for planetary surface exploration—entry, in situ systems, surface mobility, communications, and Earth-return systems (Table 8.3).

The two most-constrained resources in the current generation of planetary spacecraft are onboard power and propulsion. Improvements in these two areas will enable the largest leaps forward in capability. Solar power is generally insufficient beyond the asteroid belt, provides limited power for spacecraft systems, and severely limits the lifetime of landed spacecraft. Most solar-powered planetary spacecraft have only a few hundred watts of

TABLE 8.3 Recommended Technology Developments

Category	Recommended Development
Power	**Advanced radioisotope power systems, in-space fission-reactor power source**
Propulsion	**Nuclear-electric propulsion, advanced ion engines, aerocapture**
Communication	**Ka band, optical communication,** large antenna arrays
Architecture	**Autonomy,** adaptability, lower mass, lower power
Avionics	**Advanced packaging and miniaturization,** standard operating system
Instrumentation	**Miniaturization,** environmental tolerance (temperature, pressure, and radiation)
Entry to landing	**Autonomous entry, precision landing,** and hazard avoidance
In situ operations	Sample gathering, handling, and analysis; drilling; instrumentation
Mobility	**Autonomy;** surface, aerial, and subsurface mobility; hard-to-reach access
Contamination	Forward-contamination avoidance
Earth return	**Ascent vehicles,** in-space rendezvous, and Earth-return systems

NOTE: Bold type indicates a priority item.

power available for science. In-space chemical propulsion has limited capability, especially for missions to the outer planets, resulting in very long flight times and often limiting missions to rare launch windows requiring multiplanet flybys to gain the necessary energy. The solution to the power and propulsion problems is development of advanced nuclear power sources and in-space nuclear-electric propulsion. Advanced radioisotope power systems (RPSs) are required to replace the depleted inventory of first-generation RPSs. Advanced RPSs are required for both spacecraft power and for early low-power versions of in-space nuclear-electric propulsion (NEP). Finally, a compact and efficient (high thrust-to-mass ratio) flight-qualified nuclear-fission reactor should be developed in parallel with the development of second- and third-generation ion drives for the high-power NEP systems required to reach the outer solar system. Development of aerocapture as a means to reduce in-space propulsion requirements will significantly improve mission performance to all planets with atmospheres.

The SSE Survey is highly supportive of NASA's nuclear power and in-space nuclear propulsion initiative. The Survey believes that in the second half of this decade this program can produce advanced flight-qualified RTGs that could be flown on the Europa Geophysical Explorer and Jupiter Polar Orbiter with Probes, and on the Mars Science Laboratory. The development of in-space NEP, including its first qualification flight in space, will take almost the entire decade and will become available for advanced outer-planet missions at the beginning of the next decade. The outer-planet missions recommended for flight in this decade (e.g., the Kuiper Belt-Pluto Explorer) can be accomplished without NEP.

The development of nuclear technologies, while clearly enabling for many planetary missions, will be controversial in their application and in the public mind. This new initiative was announced too late for the SSE Survey to assemble all the required expertise and to consider all the ramifications of the proposal. The fission-based technology will take a decade to develop in any case, so the Survey devised a flight program for the next decade that does not require it. In the meantime, **the SSE Survey recommends that a series of independent studies be undertaken immediately to examine the scientific, technical, and public issues involved in the use of nuclear technologies on planetary spacecraft.** A science study should be conducted to determine which mission types are enabled by nuclear technologies and which are not. An engineering study should be undertaken to consider the design and safety aspects of the proposed nuclear technologies. And, a study should be conducted to examine public attitudes toward this technology, how to provide the public with an understanding of the issues, and means for mitigating public acceptance problems that are due to fear and misunderstanding of these issues.

In the area of spacecraft communications, it is assumed that current development of Ka-band capability and antenna arrays will mature in the early years of this decade. The next most important step is the development of optical communications for a major leap forward in communications bandwidth, particularly for video-rate communications from Mars and for advanced exploration in the outer solar system. **Advanced optical and/or radio communications should be developed and flight-qualified toward the end of this decade for use by Mars Sample Return and the next generation of outer-planet missions powered by NEP.**

In the area of spacecraft systems, the key demand is for considerable autonomy and adaptability through advanced architectures. Lower-power, lower-mass spacecraft need to be developed commensurate with realistic cost and performance for the available expendable launch vehicles. Not unrelated is the need for more capable avionics in a more highly integrated package through advanced packaging and miniaturization of electronics and with a standardized software operating system.

New and increased science measurement capability in planetary science instruments and greater environmental tolerance will be required for less mass and power. Miniaturization is the key to the reduction of mass and power requirements. For the inner solar system, electronics tolerant to extremes of temperature (both hot and cold) are required. High-temperature, corrosion-resistant, and pressure-tolerant systems are required for in situ exploration on Venus. For the outer planets, radiation-hard electronics, shielding, tolerance, and reliability are required.

As planetary exploration moves into the new century with more in situ and sample-return missions, it will be necessary to develop planetary landing systems, in situ exploration systems, and Earth-return technologies. The key requirements for landing systems are autonomous entry, descent, hazard avoidance, and precision landing systems. Once on the surface, sample gathering and analysis become key technologies, with attendant requirements for new surface science instruments, including biological measurements, and means for moving about a planet—on, above, and below the surface. Systems for accessing difficult-to-reach areas will be required.

Rover technology should advance toward long-life and long-range capability, with autonomous hazard avoidance and the ability to operate on large slopes. Drilling techniques on both terrestrial and icy surfaces will be needed, advancing toward deep-ice penetration and submarine exploration in subsurface oceans. Aerial platforms for Mars and Venus will be required; they will be the forerunners of systems to be deployed on Titan and the outer planets. Advanced autonomy will need to be built into all of these mobile mechanisms.

The means to return planetary samples needs to be developed, beginning with small bodies and the Moon, advancing toward Mars, then Venus, and eventually to more distant targets such as Mercury and the satellites of the outer planets. Some recommended missions will be sent to planets and satellites that are targets for biological exploration and will require meeting planetary protection requirements related to forward and back contamination. Technologies will be required to meet these requirements while reducing the costs to do so.

Mission-Specific Technologies

In addition to the generic technologies described above and summarized in Table 8.3, mission-specific technologies are required for the flight missions selected for this decade. They are described below.

Kuiper Belt-Pluto Explorer

The Kuiper Belt-Pluto Explorer mission is ready now, has no requirements for new technology, and can use one of the few remaining first-generation RPSs This is a multiple-object flyby mission designed as the first reconnaissance of a number of Kuiper Belt objects, including the largest and best studied example, Pluto-Charon. It is premature to consider an orbiter for any of these objects. For this reason, and because of the low relative flyby velocities required and the requirement to reach Pluto at the earliest possible date, an NEP option with the necessary advanced ion engines is not appropriate. There is no confidence that both can be developed in time, nor are they necessary for this mission. Consideration should be given, however, to the use of a solar-electric propulsion stage to avoid reliance on a singular Jupiter gravity-assist opportunity in 2006.

Europa Geophysical Explorer

Radiation-hard electronics is the key requirement in addition to the generic technologies for outer-planet missions given above. This mission is focused almost exclusively on Europa, where it is much easier to confirm the existence of a subsurface ocean and to determine its extent than it is at Ganymede or Callisto. This orbiter mission would not benefit significantly from NEP because of the strong focus on a single object with a limited set of scientific measurements. Once confirmed on one Galilean satellite, a follow-on mission might be considered using an NEP spacecraft to consecutively orbit all three outer Galilean satellites to search for the extent of subsurface oceans and to dispatch landed probes.

South Pole-Aitken Basin Sample Return

The SPA-SR mission to the farside of the Moon could be the first test of sample-return technologies to be used on Mars. The developments required for these missions are very nearly the same, except for the system for braking from orbit. The common elements are automated descent; hazard avoidance and precision landing; advanced in situ sampling, perhaps even drilling; advanced in situ instrumentation, including radiometric age-dating and chemical and mineralogical analysis; sample transfer; and an ascent vehicle and Earth-return system. A means for communication with a lunar farside station will be required. A successful SPA-SR mission will provide early demonstration of planetary sample-return technology without the need for planetary protection and will significantly reduce the risk for a Mars sample-return mission.

Jupiter Polar Orbiter with Probe

The JPOP mission will require advanced RPSs, radiation-hard avionics, and the revival of the Jupiter entry-system technologies first developed in the 1970s. The probes should survive and be in communication to 100 bars, whereas the signal from the Galileo probe was lost at 22 bars. Lightweight mass spectrometers for sampling at high pressures with internal gas processing for complex analysis are the key science instrument technology. The deep probes developed for this mission will then be available for similar missions to the other giant planets, Saturn, Uranus, and Neptune. NEP is not required for this mission.

Venus In Situ Explorer

The key technologies for the VISE mission are those for system survivability, shallow drilling, sample acquisition, and sample transfer at extreme high temperature and pressure in a corrosive environment; high-temperature balloon materials; and long-lived compact power sources. The mission will require in situ instruments that can survive the Venus surface environment and that can accomplish radiometric age-dating and chemical and mineralogical analysis of surface samples while at altitude. The use of advanced solar-electric propulsion coupled with aerocapture would markedly increase the performance of this mission.

Comet Surface Sample Return

The key technology required for the CSSR mission is a sample-acquisition system without significant on-surface time, drilling, or sample manipulation and storage at cryogenic temperatures. Advances in automation, ion propulsion, and solar- and/or nuclear-power sources will improve the performance of this mission.

Mars Missions

In addition to the generic orbital, in situ, and sample-return mission technologies listed above, for which Mars is a prototypical benefactor, planetary protection technologies (both forward and back) and attendant sample containment, Earth return, and handling and examination facilities are the key technical issues to be addressed. A Mars-Earth return system, including an ascent vehicle and in-space rendezvous and sample capture, are key technologies that can evolve from the vehicles developed for the South Pole-Aitken Basin Sample Return mission.

Technologies for the Following Decade

Technology development necessarily precedes flight-mission development, and the technologies developed for this decade must evolve into the technologies required for missions early in the next decade. The most important of the technologies developed in this decade for use in the next are advanced in-space NEP and spacecraft nuclear power systems. These power and propulsion technologies will enable missions that cannot otherwise be accomplished. NEP will reduce or eliminate the need for gravity assist, enable launch in any year, yield shorter trip times for many types of missions, reduce launch vehicle requirements, enable tours of many different destinations on the same mission, and enable outer-planet orbiters with long life, propulsion for extensive system touring, high power output, and significantly larger payloads. Active remote-sensing instruments, including synthetic-aperture radar and laser-activated techniques, will be enabled by fission power sources.

Examples of missions following naturally in the next decade from those recommended in this decade, and which are enabled or enhanced by NEP, include a Neptune Orbiter carrying Neptune atmospheric probes and Triton surface probes, a Titan Explorer mission carrying an aerial vehicle and landers for Titan, and a Saturn Ring Observer for maneuvering above Saturn’s ring plane. The addition of aerocapture technology to these missions will yield a combination of enhanced capabilities, reduced launch vehicle requirements, and/or reduced in-space propulsion system requirements.

Optical communications, including advanced science instrumentation to utilize the increased bandwidth, should be available for missions in the next decade. The perfection of Mars sample-return technology should be followed by its adaptation for return of samples from the surface of Venus. Drilling and cryogenic sampling will be required for the return of a completely preserved core sample of a comet nucleus. Aerial vehicles will be required for the exploration of Titan, Mars, and Venus; subsurface vehicles for Mars and perhaps Europa; and complex organic chemistry and microbiology laboratory packages for exploring organic-rich environments, including Europa and Titan and perhaps even subsurface aquifers of Mars. Long-lived, high-temperature, and high-pressure systems will be required for Venus sample return and surface stations such as seismic networks.

The Deep Space Network

The Deep Space Network (DSN) is suffering from insufficient communications capability and occasional failures as it ages. Limitations on downlink bandwidth restrict the return of data from spacecraft ranging from some Discovery flights (e.g., the Deep Impact encounter sequence requiring real-time links) through the Flagship Cassini mission (constrained by the feeble signal from distant Saturn). While efforts to increase the transmitter power on spacecraft are valuable, likely it will be less expensive to augment both transmitter power and communications capacity on Earth than to correspondingly increase these factors on all spacecraft. Furthermore, additional ground stations would be valuable to provide geographic redundancy for the system as a whole, and they would grant more freedom in the timing of critical spacecraft events. Studies should consider whether it is better to move toward shorter wavelengths such as Ka band, toward very large collecting areas, or toward optical communication links. Studies should also examine the efficiency gains that might be realized by using a packet-switched network protocol for communicating with a large number of planetary spacecraft.

The SSE Survey recommends upgrades and increased communications capability for the DSN in order to meet the specific needs for this program of missions throughout the decade, and that this be paid from the technology portion of the Supporting Research and Technology (SR&T) line rather than from the mission budgets. While it is perfectly reasonable, under full cost accounting, to use a straightforward algorithm that assesses costs for operating the DSN to specific missions, any upgrade cannot realistically be charged to the first mission that uses it, and an amortization schedule would be entirely ad hoc given the uncertain number of prospective client missions that might employ the DSN. Such a voluntary system of payment would make the financial status of the entire upgrade program unstable, since the program would be subject to the financial decisions of individual mission managers.

EARTH-BASED TELESCOPES

NASA currently provides support, in widely varying percentages, for planetary science operations at Arecibo, Goldstone, Keck, and the Infrared Telescope Facility, in collaboration with the National Science Foundation (NSF), DSN, a private consortium, and NSF, respectively. As described in Chapter 6 of this report, these facilities have made major contributions both to planetary science in general and to specific flight missions. The IRTF, the only facility dedicated to NASA planetary astronomy, has provided vital data in support of flight missions. **The SSE Survey recommends that the planetary radar facilities, the Infrared Telescope facility and NASA support for planetary observations at large facilities such as Keck be continued and upgraded as appropriate, for as long as they provide significant scientific return and/or provide mission-critical service.**

The recent so-called Augustine report urged that NASA and NSF collaborate in astronomy in order to coordinate their efforts and produce the best science for the national investment.[3] In particular, that report's second recommendation urged the federal government "to develop a single integrated strategy for astronomy and astrophysics research that includes supporting facilities and missions on the ground and in space."[4] The SSE Survey notes, however, that developing such a single, integrated strategy for planetary astronomy will not be easy. While NASA's support for the Keck and IRTF facilities on Mauna Kea has been enthusiastic and substantial, there appears to be growing reluctance to fund some kinds of ground-based astronomical research. Similarly, NSF has provided very limited support for planetary science in recent years, a situation that is particularly unfortunate, given NSF's charter to support the best science and its leadership role in other aspects of ground-based astronomy.

While the SSE Survey presumes that the Solar System Exploration program's current collaborations with NSF and private consortia will continue as long as they are scientifically productive and relevant to NASA's missions, it notes that the coming decade presents a nearly unique opportunity to develop better coordination and collaboration, particularly in light of significant overlap between recommendations of this survey and those of the 2001 astronomy and astrophysics decadal survey.[5]

In the spirit of the Augustine report's second recommendation, **the SSE Survey recommends that NASA partner equally with the National Science Foundation to design, build, and operate a survey facility, such as the Large Synoptic Survey Telescope (LSST) described in *Astronomy and Astrophysics in the New Millennium*, to ensure that LSST's prime solar system objectives are accomplished.** The particular planetary objectives of LSST are as follows:

- Determine the contents and nature of the Kuiper Belt to provide scientific context for the targeting of spacecraft missions to explore this new region of the solar system;
- Assess the population of near-Earth objects (NEOs) down to 300-m in diameter and provide a measure of the impact hazard; and
- Ascertain the relative importance of long-period comets as impact hazards to Earth.

The LSST (Figure 8.1) will also assess the distribution of Centaurs and search for uranian and neptunian Trojans. Such a facility has been separately recommended by the most recent astronomy and astrophysics decadal survey.[6] The latter report lists NEO detection and Kuiper Belt object surveys as LSST's two top science drivers, followed by a host of astrophysical applications. Indeed, the parameters of the LSST are largely determined by the need to detect NEOs, since this is the most difficult measurement to make with the telescope.

The design of missions to the small bodies of the solar system requires extensive physical characterization of a significant subset of these objects in order to properly choose the best targets to answer particular scientific questions. This physical characterization is best done with telescopes having a suite of instruments for imaging and spectroscopy at various wavelengths. While the brighter of the small bodies of the solar system can be readily studied with what are now thought of as small to medium telescopes, the fainter members of the Kuiper Belt, which are orders of magnitude more numerous than the bright members, cannot be characterized with existing facilities.

Similarly, assessment of the hazard from NEOs requires physical characterization of the ensemble by remote sensing in order to carry out the missions to investigate more detailed physical characteristics in situ. As with the Kuiper Belt objects, the fainter NEOs and long-period comets require a very large telescope for physical characterization.

The high-angular-resolution capability of large ground-based telescopes equipped with adaptive optics (AO) now surpasses that of telescopes in space. For example, the Keck and Gemini telescopes routinely achieve angular resolutions better than 50 milliarcseconds (mas) at near-infrared wavelengths. Planned ground-based telescopes will have resolutions better than 10 mas. At this resolution, the disks of Jupiter and Neptune can be resolved into 10^7 and 4×10^4 resolution elements, respectively, opening the intriguing possibility for long-term studies of atmospheric dynamics and spectroscopy from the ground. Spectroscopy of the giant planets is crucial for understanding the altitude variations of their atmospheric properties.

The requirements of a telescope capable of performing the physical characterization of small solar system bodies described above—a 30-m-class, fully steerable facility equipped with adaptive optics—are similar to those of the Giant Segmented Mirror Telescope (GSMT) as proposed by the 2001 astronomy and astrophysics decadal survey (Figure 8.2).[7] This telescope will allow characterization of 10-km bodies in the Kuiper Belt and allow targeted searches for 1-km objects that are inaccessible by other means. It will permit continuous study of the atmospheres of the planets as a precursor and complement to the missions prioritized in this report. The planetary community should be fully involved in defining the capabilities of the GSMT, including its all-important AO system and the specific instruments that will be developed for this telescope.

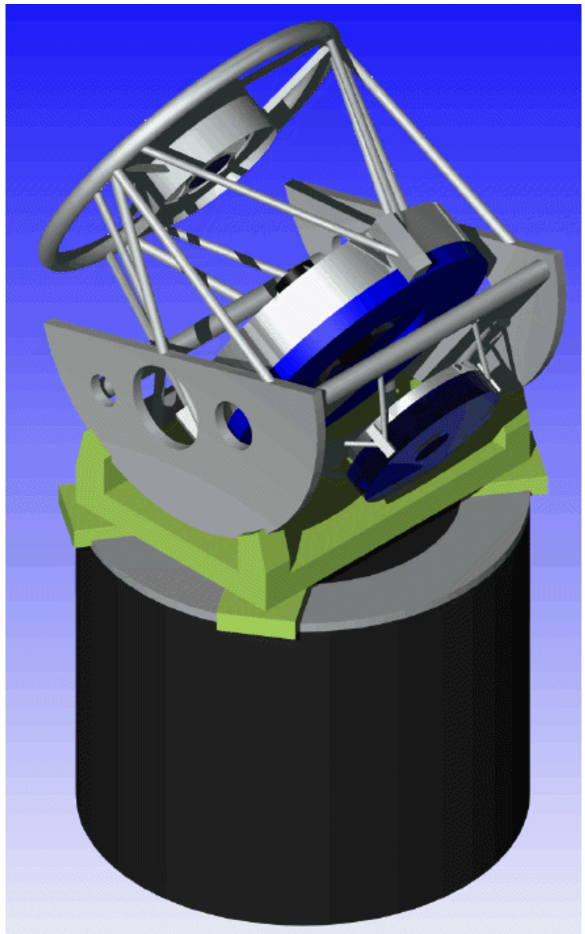

FIGURE 8.1 An artist's impression of one particular concept for the Large Synoptic Survey Telescope. Courtesy of the National Optical Astronomy Observatories.

The SSE Survey endorses the 2001 astronomy and astrophysics decadal survey recommendation for a Giant Segmented Mirror Telescope and further recommends that it be utilized for the physical characterization of solar system objects.

The track record of contributions to solar system exploration by Earth-orbital missions sponsored by the other themes at NASA has been exceptional and was made possible only by ensuring that those facilities have an appropriate capability to track moving targets. The James Webb Space Telescope (JWST) clearly has the capability to make major contributions as long as it is provided with the capability to track moving targets. **The SSE Survey recommends that capabilities particular to planetary science (e.g., the need to track non-sidereal objects) be incorporated into the James Webb Space Telescope as fully as possible in order to maximize the science return.**

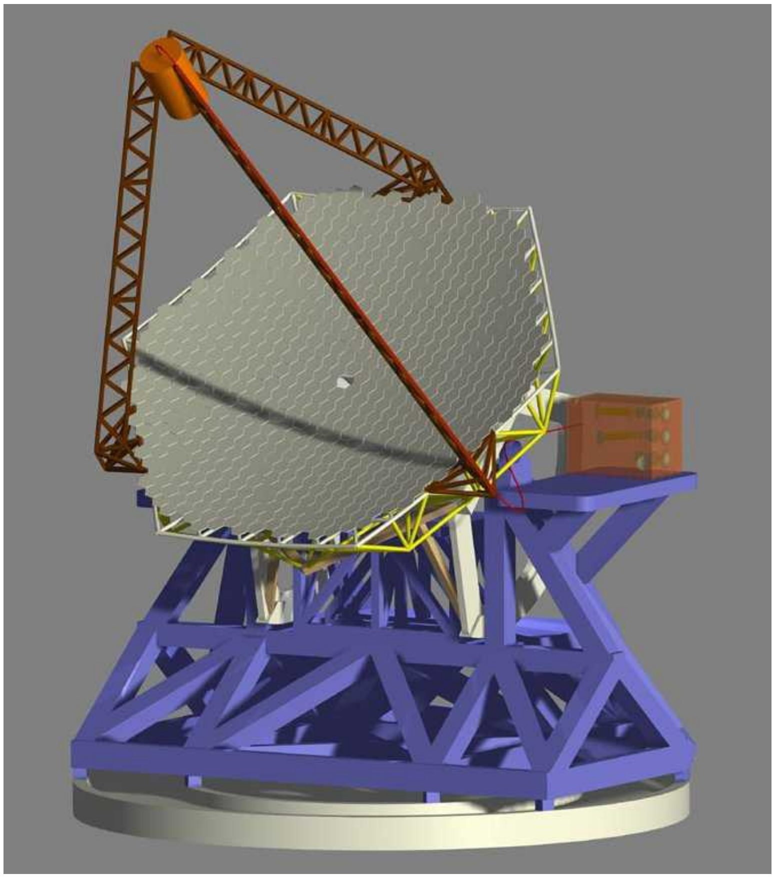

FIGURE 8.2 An artist's concept of one particular configuration for the proposed Giant Segmented Mirror Telescope. Courtesy of the National Optical Astronomy Observatories.

REFERENCES

1. Executive Office of the President of the United States, *Budget of the U.S. Government—Fiscal Year 2003*, U.S. Government Printing Office, Washington, D.C., 2002. Available online at <http://www.whitehouse.gov/omb/budget/fy2003/budget.html>.

2. Space Studies Board, National Research Council, *Assessment of Mars Science and Mission Priorities*, National Academies Press, Washington, D.C., 2003.

3. Space Studies Board and Board on Physics and Astronomy, National Research Council, *U.S. Astronomy and Astrophysics—Managing an Integrated Program*, National Academy Press, Washington, D.C., 2001.

4. Space Studies Board and Board on Physics and Astronomy, National Research Council, *U.S. Astronomy and Astrophysics—Managing an Integrated Program*, National Academy Press, Washington, D.C., 2001, p. 4.

5. Board on Physics and Astronomy and Space Studies Board, National Research Council, *Astronomy and Astrophysics in the New Millennium*, National Academy Press, Washington, D.C., 2001.

6. Board on Physics and Astronomy and Space Studies Board, National Research Council, *Astronomy and Astrophysics in the New Millennium*, National Academy Press, Washington, D.C., 2001.

7. Board on Physics and Astronomy and Space Studies Board, National Research Council, *Astronomy and Astrophysics in the New Millennium*, National Academy Press, Washington, D.C., 2001.